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NITROUS OXIDE AS AN ANÆSTHETIC.

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Nitrous Oxide, as an agent for assuaging pain by producing temporary insensibility, is of recent date. The occasional blunders which accompany the early introduction of every valuable application of science have, in the case of nitrous oxide, been met with a spirit of unfairness and severity scarcely equalled; while every effort has been made to persuade the public mind that the failures of the inexperienced are due to the thing itself, not to its abuses. Travelling mountebanks, unskilled in the chemistry of this compound, have been too often guides for the unpractised operator; and from the exhibition of "fools superexcited into hysterical demonstrations," erroneous ideas of its properties and applications have been gained.

The superiority of nitrous oxide for dental anæsthesia, when properly made and administered, is susceptible of the clearest proof.

1st. It can be furnished at less than one fourth the expense of ether. With an apparatus where the heat shall be accurately and uniformly regulated by the flow of the nitrous oxide itself, precluding the possibility of overheating the nitrate of ammonia, a pure gas, unwatched, may be prepared at a comparatively trifling cost. This has been recently effected.

2d. It is without injury to the operator. Every dentist who administers ether or chloroform, is sensible of the injurious results of an atmosphere pervaded by the vapor of such anæsthetics. In the use of nitrous oxide this inconvenience is never felt.

3d. It is safer for the patient. Motives of self-interest, or a desire to shift the responsibility of failure, have led some, even in the dental profession, to decry the use of nitrous oxide as fraught with danger. Such assertions have been too often and too thoroughly refuted to require any lengthy reply. We have repeatedly given the gas freely for sanitary purposes where ether was pronounced

unsafe, and never with the slightest unfavorable results. At a dental office where the gas has been made with care and extensively given, we are assured that of some four thousand cases where it has been employed, not one has shown the least injury resulting to the system. Several well-known dentists who now employ the gas, assure us that cases of temporary sickness, which used frequently to follow the inhaling of ether, now, since the use of the gas, never or very rarely occur.

Injury may result from the wrong use of any agent. In therapeutics how many safe and invaluable remedies in experienced and skilful hands, prove poisons when prepared and dealt out by ignorant tyros. Safety depends on the right use of means. God never suspends the laws of the physical system to accommodate human ignorance. What more harmless than water! And yet a wrong use has in many an instance proved fatal.

In the use of nitrous oxide, proper materials, and a right use of them, are guarantees of success. Parsimony, which tampers with life and health by employing a corrupt article of nitrate of ammonia, a half-washed gas, and unsuitable appliances for administering this, should charge the fault of failure where it belongs—on the user, not on the thing used.

Chemically pure nitrous oxide, rightly and liberally administered, we believe the most harmless of anæsthetics yet discovered. Such, with pure ammonia and the late improved facilities for heating this and washing the gas, may be easily and cheaply prepared in every dentist's office.

Attempts have been made, by interested parties, to prejudice the public against the use of nitrous oxide, by representing the ordinary mode of inhaling it as "filthy and disgusting." Such a puerile objection we deem unworthy a reply, and should pass it, but for the fact that it is not without its influence with morbid and unreflecting minds. That there are slatterns in dentistry as in culinary offices, is not a question of doubt. Who is so simple as to decry all catering, because the dishes of certain hotels have been found in some cases improperly rinsed? The same idea holds true in the use of nitrous oxide. But admitting, if you please, the return of the breath to the bag as neither safe nor cleanly (which we deny where the quantity of the gas is sufficient), the objection may be readily met by a use of the valved breathing tube attached to the cock of the bag, allowing no return of the breath to this. Such we have tried with the best of results—producing perfect anæsthesia, with a moderate quantity of the gas.

Careful, patient experiment is the only proper basis for chemical reasoning. On such we have endeavored to found our views, as expressed above. Nor will the fulminations of wordy theorists at either extreme of the chemical ladder shake us from a belief in well-attested facts. A. W. S.

DR. GAILLARD'S PRIZE ESSAY ON OZONE; ITS RELATIONS TO HEALTH AND DISEASE.

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THE same experiment repeated with distilled water, gives as a result number 2 of the ozonometric scale. If the distilled water be boiled so *as to expel all contained atmospheric air*, the ozonoscope remains unchanged. This proves that, in this process, the ozône is not formed from the oxygen escaping during the decomposition of the water by evaporation, but from the oxygen contained in the water. The oxygen is converted into ozone (in the presence of water) by the electrical and electro-chemical reactions which always attend the process of evaporation. If the water is made to give up all of the oxygen mechanically contained in it, ozone is not manifested during its evaporation. Distilled water, unboiled, contains a little air, enough to give number 2 of the ozonometric scale. To make these experiments more conclusive, the surface of the water was covered with oil, so as to prevent all connection with the atmospheric air. If light, during the experiments, be destroyed, the result is immediately changed. When a glass receiver is used and rendered non-diaphanous, ozone is not formed; a cloth thrown over the glass will prevent its formation; sometimes the prevention is complete; the diminution in the quantity formed is invariable. This is a most interesting fact to the student of hygiene; it is a beautiful and suggestive method of demonstrating the effect and necessity of light. Ozone will not be formed if this experiment is performed at night. It will not be formed, either in the day or night, if artificial heat be substituted for that of the sun. In the formation thus of ozone, there must be exposure to sunlight. When salt water is used, the results are the same. Rain water is most active in the genesis of ozone; it is well known that this contains the largest quantity of atmospheric air. Most of these experiments were originated and performed by M. Scoutetten. These are the most important and interesting results demonstrated in the investigation of this branch of the subject.

Without any labored appeal to facts and mathematical statistics, we can realize the stupendous extent to which this agent is generated when the sun shines down on the vast areas of sea and lake and river which form so large a portion of our globe. It has long been supposed that the pine tree exerted a chemical effect (as it is termed) upon the poison of malaria; it is interesting to find, in the existence of ozone in such atmospheres, a plain and simple explanation of this undoubted fact. As a rule, almost without any material exception, malaria does not prevail in the dry atmospheres of the pine forest. It is to such atmospheres that thousands resort during the summer months at the South, for immunity from malarial pestilence. What is the explanation? It will be recollected that ozone was manifested in the air of a vessel containing turpentine, if the ves-

sel be exposed to the agencies of sunlight. The ozogenetic properties of the pine tree have never been mentioned by any one; yet does not reason teach, that for the production of this great atmospheric purifier (ozone) the pine forest is one of the most active agents in the extended laboratory of nature? It has always so seemed to us, and we offer the suggestion for consideration. The balmy influences of these terebinthinate atmospheres are thus reasonably explained, and the cause of their general freedom from malaria suggested. All rooms newly painted, in which turpentine has been made the vehicle of the pigment, give a deep hue to the ozonoscope. The same principle, it is claimed, must necessarily pervade the atmosphere of the pine forest. We submit the suggestion, however, without farther reasoning, for general consideration. We pass on now to consider ozone as formed by the electrization of oxygen, given off by vegetation; this being the last, but a most important natural source of this agent.

Scoutetten has announced, after many careful experiments, that plants do not give off oxygen during the day, as declared by Priestley first and many others after him; *but that this gas is ozone.* His experiments for proving this novel and original theory are at least ingenious and plausible. They deserve close examination and criticism, and, if invariably true in their results, cannot be too generally known. This laborious philosopher should receive all possible credit for his originality, enthusiasm and energy.

If a bell glass receiver, with an ozonoscope attached to its inner surface, be placed over vegetation, it is soon filled with a gas which gives a blue color to the ozonoscope. No one claims that oxygen can *possibly* produce this result. To prevent the possibility of terrestrial electricity influencing the result, the plants were suspended by silken strings. The ozonoscope usually registered 5 or 6 of the scale. Ozone was not thus formed, if the experiment was made at night, or if the sun-light was obstructed. This experiment would very prettily demonstrate the actinic agencies of sunlight.

It was important, in this experiment, to discover whether ozone was formed from the oxygen attending evaporation from the surface of the leaves, or whether it was due to the chemical and vital actions taking place in the plant. This was determined by inverting two glass receivers over water and vegetation respectively. The receiver over the water gave no evidence of ozone, whilst that over the vegetation soon became filled with a gas which discolored the ozonoscope. This experiment proves, what has already been proved, that ozone is not formed from the oxygen of evaporation; it also proved, that ozone is formed during the day, from the chemical and vital actions taking place in plants. It was thus demonstrated, that plants evolve ozone when under the influences of sunlight; that this action ceases at night, in artificial obscurity, and where the plants do not receive the direct rays of the sun. If a glass receiver is inverted over dried

earth deprived of vegetation, the ozonoscope remains unaltered. These facts established, we can appreciate, if not realize, the vast amount of ozone discharged from the vegetating surfaces of the earth. When reflecting upon its various other sources, we cannot but feel the important part it must play on the health and well-being of humanity. We will now consider the ozonoscope, in its importance, nature and manufacture.

As soon as ozone was discovered by M. Schönbein, he sought for some reliable means of detecting its presence. After various experiments, he selected the iodide of potassium as being best adapted to this purpose, and combined with it some substance for manifesting the amount of decomposition taking place in it, when subjected to the influences of ozone. This substance is starch. Many other tests have been suggested; some ingenious and delicate, but none answering so well as the test selected by M. Schönbein. In making the ozonoscopes, it is essential that the iodide of potassium should be *absolutely pure. The commercial preparations are totally unfit for experimental purposes,* and only obscure and vitiate the results when used, the experiments being not only defective, but the deductions altogether false. The *best* commercial preparations are adulterated, and although not sufficiently so to render them unsuitable for therapeutic purposes, still they are totally unfit for the laboratory. Those designing to work in this field should obtain their instruments from Germany, or France; the ozonoscopes are manufactured there with great care. It is very difficult to obtain, in America, a perfectly pure preparation of the iodide of potassium. Of course there are many laboratories where it might be manufactured, and our pharmaceutical chemists could prepare as good an article as could be obtained anywhere; but the important fact to be recognized is, that our laboratories do *not* produce a good preparation, and our chemists do not feel sufficiently interested to supply what is wanted for such purposes. There is one source for obtaining a perfectly pure article, in the United States, which should be known. This is at the U. S. Navy Laboratory attached to the U. S. Hospital near the Navy Yard at Brooklyn, Long Island. At this laboratory (under the charge of Dr. Beach, an accomplished chemist) the Government manufactures all preparations intended for the Navy. Dr. Beach, we are authorized to say, will always take pleasure in furnishing a perfectly pure KI, when authorized by the Secretary of the Navy to do so. It is superfluous to say that this permission can always be readily and immediately obtained; and thus one great obstacle and inconvenience be removed.

The ozonoscopes are thus prepared:—℞. Potassii iodid., gr. i.; amyli pulv., gr. x.; aquæ dist., gr. c. (by weight). The starch should be free from extraneous matter, and the water always distilled; rain water will not answer, as it frequently contains nitric acid, and this would injure the preparation. The iodide of potassium should be

dissolved in the water; the pulverized starch added to the solution, and the mixture placed in a porcelain vessel, held over a "slow fire." Stir the whole, with a *glass rod,* and when it gelatinizes, set it aside until it is cold. This paste is then spread on white paper, which, when it is dry, is cut into slips, two inches long and half an inch wide. In spreading the paste on the paper, it is best to use a wooden knife, as metal may contain some impurity that would injure the preparation. These papers should then be carefully placed away, and not roughly handled, *for the paste scales off* and carries with it the iodide of potassium. Bibulous paper was first used, but Berzelius pointed out its constant imperfections, and white glazed paper has been substituted. Chlorine (or other bleaching agent) gives to the test paper a slightly red tint, from the chemical reactions caused in the iodide of potassium. There are other objections to the use of paper for the ozonoscope, and most observers have in a great measure abandoned it. In its place is used a material which fully accomplishes all that could be desired. This is cotton muslin; and when properly prepared, there can be no difficulty attending its use. Ordinary bleached muslin, of the best description, should be obtained, and carefully washed and re-washed in hot distilled water, to remove any chlorine or lime that may be present. It is then torn into pieces, about three inches wide and of length corresponding to the original width of the muslin. Steep these pieces, for twelve hours, in a solution of the iodide of potassium and water, one grain of the iodide of potassium to one hundred grains (by weight) of distilled water. When fully dried, spread with a wooden knife upon one side of the cloth, the mixture of the iodide of potassium (one grain), starch (ten grains), and distilled water (by weight one hundred grains). When this mixture has nearly dried, let the pieces of cloth be carefully "ironed," on the sides unprepared. They may then be cut into slips, three inches in length and one inch in width, and carefully placed in glass bottles for use.

At the Leeds meeting of the British Association, in 1858, Dr. Lankester exhibited his instrument for the convenient disposition of the ozonoscopic cloth and for registering, hourly, the amount of atmospheric ozone; it is so exceedingly simple, yet efficient, that most observers have adopted it. This instrument moves like a clock; and on a shaft, in direct connection with the machinery, is wound the ozonoscopic cloth. The instrument in its action unwinds a given length of cloth during every hour, and this piece, so unwound, is kept constantly exposed to the atmosphere. By a convenient and compensating arrangement another shaft, also in connection with the instrument, winds up this cloth as it is unwound from the first shaft. The cloth, as it slowly passes from one shaft to the other, is made to move before an aperture in the side of the box and is there exposed to the atmosphere. The cloth should be kept damp during its exposure. Some very simple method will answer; a piece of soft cotton

thread, or cord, with one end resting in a vessel of *distilled* water and the other placed in contact with the ozonoscopic cloth, so that by capillary action the water will pass from the vessel to the cloth—or any other method that the most ordinary ingenuity would suggest. An ordinary clock will answer the purpose of Dr. Lankester's apparatus, and is always easily attainable. It will only be necessary to have added some shaft, which, by being placed in connection with the machinery, will wind up the cloth as it is unwound from any *selected* shaft of the clock. The hours and half hours should be marked, on the unprepared side of the cloth, in ink, and in this way it will be very easy to tell, not only the degrees of ozone during any given time, but the exact hour or fraction of an hour at which each degree was thus registered. If one inch of cloth be exposed every hour, a division of the cloth into inches and half inches will afford a perfect index of time. Whether the cloth be cut into slips, or used in entire pieces, it should always be dampened at the time that it is compared with the ozonometer, for determining the number expressive of the chromatic shade generated. There is nothing in the atmosphere that will color this cloth but ozone. Oxygen, whether dry, damp, or heated, produces no change in it; chlorine does not exist in the air; iodine, though claimed to be occasionally present, is proved never thus to appear in an atmosphere in its normal condition; so that when this cloth is changed it must be by ozone only, and the degree of the change marks accurately the quantity of ozone present. Iodine must be in its *free* state to give the blue test with a cold solution of starch, and the liberation of iodine (when the iodide of potassium is used) is usually accomplished by adding a few drops of acid or chlorine water to the mixture. When the ozonoscope is used, ozone plays the part of an acid; it unites with the potassium, converting this into potassa, whilst the liberated iodine unites with the starch, forming the blue iodide of fecula. The depth of this blue will depend upon the quantity of iodine liberated, or, in other words, upon the quantity of ozone present.

Dr. Charles Smallwood (Professor of Meteorology in the University of McGill College, Montreal, Canada East) has instituted and published a series of beautiful experiments, serving to show the effect of different colored light upon the ozonoscope. "By exposing the ozonoscopes under different colored glasses and also to the action of polarized light, the following were the results. The expression 1.00 being for saturation gives the ratio exposed, to

Direct light	·73	Orange light	·55
Polarized light	·64	Purple light	·51
White light	·57	Blue light	·45
Red light	·58	Green light	·41

"The following table of the properties of direct light, through colored media, is given for the sake of comparison:—

Medium.	Light.	Heat.	Chemical Action.
White	7	7	7
Red	4	5	6
Orange	6	6	4
Purple	3	4	6
Blue	4	3	6
Green	5	2	3

The whole sunbeam consisting of luminous rays, heating rays and chemical or actinic rays, light therefore, passing through the colored media given, becomes deprived of one or more of these properties. In submitting the ozonoscopes to these colored rays, it is shown that light passing through a green medium prevents the formation of ozone, in the proportion of 41 to 73. Polarized light would seem to possess the least influence on the development of ozone; it gives the proportion of 64 to 73. Next comes white light, then red, orange, purple, blue and green. Green, it may be remembered, possesses but half the chemical or actinic action of red, purple and blue. Orange, which possesses the greatest amount of luminous and heat rays, gave ozone in the proportion of 55 to 73." Dr. Smallwood mentions that the ozonoscopes placed, during the prevalence of the "potato rot," between the rows of the diseased plant, were much more deeply colored than those placed in the usual position. His ozonoscopes, placed among vegetables and flowers, gave no very decided results, *but he did not use the glass bell receiver during these experiments.* It is manifest that ozone may be given off from vegetation in the most *abundant* manner, but unless this gas be confined in a jar or receiver, it does not become sufficiently concentrated to produce decided effects upon the ozonoscope.

Dr. Smallwood has been long engaged in the study of ozone, and his views are at all times instructive and interesting. In a very able paper, read before the American Association for the Advancement of Science, held at Montreal, he, with many others, took the ground "that a moist and humid atmosphere was necessary to the development of ozone." This, it will be observed, is also in direct antagonism to the claims of those who assert that ozone is abundantly generated in the electrization of pure and dry oxygen. Before dismissing the subject of the ozonoscope, it will be proper to mention the different tests which may be used for demonstrating the presence of ozone.

The ozonoscope, saturated with a weak solution of sulphate of manganese, becomes brown when exposed to ozonized air. Spirituous tincture of guaiacum is turned blue by ozone. Sulphide of lead is whitened by it. Sulphate of indigo in solution is bleached by it. Silver foil, in an atmosphere of concentrated ozone, is changed to peroxide and crumbles to pieces. An alkaline solution of lead has a precipitate of peroxide formed, when submitted to the action of ozone. By it all the preparations of protoxide of manganese are

changed to peroxides. Sulphurets of lead, copper, iron and antimony are converted by it into sulphates. Arago and Schönbein frequently used plates of platinum as tests; when ozone is present, they become negatively polarized. Bouchotte has used a novel and beautiful method of estimating the quantity of ozone in the atmosphere. Observing that silver or copper was rapidly oxidized by ozone, he placed a thin sheet of copper on acidulated water; the ozone of the atmosphere oxidizes it, and, with the acidulated water, transforms it into an acetate of copper; this is taken into solution; the liquid is then evaporated, and the salt of copper, being weighed, gives the ratio of ozone present. It is evident, however, that all of these cursorily mentioned tests are more curious than useful, and are of not a sufficiently practical character for the accomplishment of any prompt or decided result. The iodide of potassium is the only test upon which we can rely, and it is always efficient, whether in the most delicate or extended series of experiments.

With the preparation of the ozonoscope, it became essential that some method should be adopted for permanently registering the shade of the ozonoscope after each experiment; or that some scale should be adopted, each degree of which would represent a certain shade of the ozonoscope after immediate use; this shade soon changing after the experiment is completed. It is also necessary that such an instrument should be accurate and uniform, as upon its results all the calculations and deductions, relative to hygiene and meteorology, would be based. An instrument for this purpose was in time suggested, and, *although radically defective and totally imperfect*, is the only one now used by most observers. This instrument is called an ozonometer. It is formed by dividing into ten (10) shades or degrees, the chromatic interval existing between the entire absence of ozone, which is registered as white, to the deepest blue which ozone can possibly produce by the decomposition of the iodide of potassium in the presence of starch. This ozonometric scale extends from zero, which indicates the total absence of ozone, to ten (10), which represents the deepest shape of blue resulting from the decomposition occurring in the chemical constituents of the ozonoscope. We have thus ten (10) degrees or shades on the face of the ozonometer. It will readily be perceived how extremely imperfect and objectionable this instrument must be. The division of the chromatic interval, from white to deep blue, is made *arbitrarily*, and this is but saying that such a division must be *always variable and exceedingly inexact*. It is manifest, that when *no scientific rule* has ever been suggested for the accurate and uniform division of this chromatic interval; that when each observer, experienced and inexperienced, scientific and superficial, is left to his own judgment and chromatic discrimination, the shades representing the different degrees of their instruments must vary *indefinitely and interminably*. It is difficult to imagine, even, how a more ingenious method could be suggest-

ed for producing *constant discrepancy and confusion;* how greater error and fallacy could be created, than by this rude and reckless method of registering the most delicate of chemical reactions. Few persons, in actual conference, can agree as to the exact difference which, in two chromatic shades, should constitute a degree. How utterly impossible, then, does this become, when, for two persons, we have hundreds, and these not in actual conference, not even in correspondence with each other, but separated by thousands of miles, and in reference to each other totally isolated; where the subject of error and confusion is not between *two* shades only, but *ten.* How, under such circumstances, can there be harmony; how even comparative uniformity? And if these be absent, for the elucidation of the problems occurring, of what practical use is this instrument? What would be the result, if thousands were investigating the subject of heat, *each using thermometers not known to be constructed on different scales, but supposed to be exactly similar; yet not corresponding in a single degree.*

A defect in the ozonometer, now used, would of course make but little difference with the individual observer, as it is evident that his maximum and minimum would have reference to but a single locality; that the real errors would compensate each other in deducing a mean; and that his instrument, however differing from those used by others, would at least give him a fair exposition of the atmospheric changes occurring around himself. But when these instruments, *so differing with each other, yet assumed to be similar,* are used by all observers, there can possibly be nothing but discrepancy in general results; errors and antagonisms in deduction. These constant and *still existing* defects must embarrass every observer, and vitiate the entire results; though they do not in any way interfere with experiments made in reference to a single locality. If cholera marks the absence of ozone in Paris, as declared by an observer there, and attends the same atmospheric phenomena in Berlin, as noticed by similar authority in that city, the deduction is clear and satisfactory, though the instruments used may be dissimilar. But if in Paris it is declared that influenza becomes epidemic when the ozonometric record is 3, when it is only epidemic in Berlin at 1, it does *not* prove that the *same* atmospheres do *not* give the same pathologic results, but, to those familiar with existing difficulties, that the ozonometers used differ perhaps and almost certainly *two* degrees in their scale; and that the atmospheric effects are *identical.*

There has been always more or less discrepancy, when many observers have compared or published their ozonometric records, with a view to some decided deduction, or for the establishment of some law attending certain ozonic phenomena. Whilst difficulties of this kind have almost invariably characterized attempts at deductions, based *on aggregate records,* there has seldom been any trouble with any individual observer. It has been found, for instance, that a cer-

tain disease has manifested itself *repeatedly* at a certain place, as a coincidence with some ozonometric number, when, to the confusion of those interested, the same disease has prevailed at another locality when the condition of the atmosphere was entirely or conspicuously different. Here, as is manifest, the sole cause of the difficulty was not in atmospheric difference, but in instrumental imperfection and discrepancy. The ozonic statistics and records abound in instances of this kind. The published tables of Boeckel Sr., Wolf, Simonin Sr., Billiard and others, are instances of these perplexities. The number of one ozonometer that represents average health, may in another indicate the incipient appearance of disease. This has occurred. Although the discrepancies thus mentioned have not been very great, yet they have been the cause of much disappointment and confusion. The conviction, in this respect, now is, that these discrepancies are not in atmospheric peculiarities, but the results of dissimilarity in the instruments used. Although this difficulty has been universally felt and regretted, yet, so far, nothing whatever has been done to remove it.

After repeated experiments and many failures, we have perfected (as we believe and hope) an ozonometer which fulfils every purpose desired. So far as we are aware, and so far as we have been able to judge, by competent testimony on the subject, it is calculated to remove all of the past and present difficulties, in connection with this branch of the subject. It is very easily made, simple in its construction, and *insuring* uniformity in *general* results. There is a peculiar enthusiasm which always characterizes the claims and writings of those who submit such offerings as contributory to science. This enthusiasm is the fruitful cause of endless errors, and can only be entirely destroyed by the caustic touch of analysis and criticism. We have endeavored to guard against it, by presubmitting ourselves to such a remedy. Whilst the correspondence, in this connection, furnishes all the testimony we could desire, it cannot be submitted without a violation of good taste and essential etiquette. These ordinary rules of propriety and a natural loathing for the practices of the empiric, induce its entire and total suppression; yet such is our desire to do all things possible, for the honest fortification of our position, in regard to this suggestion, that, even at the risk of censure, we will introduce an extract from a single letter:—

"I must say, that the construction of a uniform scale of shades is very much to be desired, and I really do think that your method is a good one, because it is uniform and the shades constant and easily attained. I congratulate you on your ingenuity and thought on this subject, and would advise, by all means, the adoption of your method of fixing the tints," &c.—(Autograph letter from Dr. Smallwood, Professor of Meteorology in the University of McGill College, Montreal, Canada East, February, 1861.)

This extract is introduced, with the double purpose of strengthen-

ing, by all proper means, our position in regard to the ozonometer presented for consideration, and (as Dr. Smallwood's recommendation can influence many who would discard the suggestion with a *passing notice)* of inducing as many observers as possible to give the instrument a fair and impartial trial. We have introduced this extract with much reluctance, and, for obvious reasons, after much hesitation and reflection; and we feel that such a course, even with the motives given, may give rise to censure; we hope, however, that the reasons mentioned will be regarded as sufficient for the justification of the action adopted. We will briefly describe the instrument mentioned.

With the materials used, observers must of necessity have ozonometers identical in shade and degree. The scale is the same as that hitherto adopted; it has been considered best not to change it. This scale is from zero, representing a perfect white, to degree ten (10), which indicates the deepest blue, caused by the decomposition of the iodide of potassium in the presence of starch. The chief difference in the construction of the two scales is, that the old scale is made arbitrarily, and the new by a regular rule; the chromatic intervals on the scale being equally and regularly divided. In the ozonoscope, ozone plays the part of an acid; in the ozonometer, we propose that, conversely, an acid shall play the part of ozone. The materials used are the iodide of potassium, nitric acid, and distilled water. The iodide of potassium can, with proper care, be now obtained in a state of purity. There can be no difficulty in this respect. The nitric acid can be obtained from any druggist, and, by a very simple process, reduced to the condition required. In the construction of the ozonometer, the nitric acid should be of exactly 1.42 specific gravity, and this can be insured by subjecting the acid to ebullition. As purchased, the acid will never be of greater specific gravity than 1.42, though it can be made at 1.5; yet this is seldom if ever done; and if so, the acid soon absorbs moisture and sinks to 1.42. As we have mentioned, though the acid can be made at 1.5, and may, at a few laboratories, be purchased of this strength, yet the *acid of commerce* does not, in specific gravity, exceed 1.42; it is frequently less than this, but, if this be the case, the same process (ebullition) will raise it to 1.42. In nitric acid at 1.42, there is, according to Dr. Ure's table, 60.572 of dry acid in 100 parts. The acid of commerce is usually white or yellow; if the first, it consists of nitric acid and water; if the last, of nitrous and nitric acids. The yellow color is due to the presence of nitrous acid. If water is added to this mixture, the nitrous acid is decomposed, forming nitric acid and nitric oxide. The nitric oxide is given off in yellow fumes, and there is left a clear mixture of nitric acid and water perfectly white. We can, by adding water to *any* preparation of nitric acid, succeed in thus obtaining a clear and simple mixture of the nitric acid and water. Now there is this very interesting and impor-

tant chemical peculiarity about nitric acid. If it be a concentrated and a heavy acid, its density is altered in ebullition, by the loss of more acid than water; if it be a weak acid (acid and water), its density is also altered, in ebullition, by the loss of more water than acid. Thus, in this process of ebullition, a *strong acid loses acid and sinks to* 1.42; *a weak acid loses water and rises to* 1.42; for at 248 F. it distils over, of this exact specific gravity.

[To be continued.]

ON THE TREATMENT OF ALBUMINURIA IN CHILDREN.

[AT a recent meeting of the Royal Medical and Chirurgical Society a paper was read on this subject by Dr. W. H. DICKINSON, of which we copy an abstract and the discussion following it, from the *Medical Times and Gazette.*]

The granular kidney appears to be unknown in childhood. The only form of disease which produces albuminuria at this period of life is that which produces enlargement of the kidney and gives it a smooth mottled exterior. This is, in fact, a renal catarrh. The tubes become obstructed by an excess of their own epithelial growth, and hence arise all the evils of the disease. If only there is a free escape for the contents of the tubes the vascularity of the gland will be relieved by secretion, and the disorder will soon be at an end. The principle of treatment must be to send as much water as possible through the organ. This fluid is devoid of irritating properties, and probably passes through the gland rather by filtration than true secretion. With these views the patients were restricted to a fluid diet. They took from two to four pints of distilled water daily, and small doses of the infusion of digitalis. When the active symptoms had subsided iron was given. Twenty-six cases were adduced in which this treatment had been pursued. Twenty-two recovered completely; three were lost sight of while improving, but while still having a small quantity of albumen in the urine; one case did badly, and eventually died under other treatment. Many of the cases were of great severity. These results appear better than those afforded by other methods. Among the in-patients at the Children's Hospital otherwise treated, 11 died out of 39; and of 69 cases treated by Dr. Miller in Dispensary practice, 8 died. It was found that on an average the little patients were restored to apparent health in 30 days, while 15 days more were needed to get rid of the last traces of albumen. The use of the water did not seem in any case to increase the dropsy, but the contrary. It was usual, however, when the swelling was great to let the digitalis set up a certain amount of diuresis before ordering the full quantity. The subsequent use of iron was believed to correct the effects of the disease, without influencing the disease itself. On the occurrence of secondary disorders, such as convulsions, or acute inflammatory attacks

it was argued that the treatment of the renal mischief should be sedulously persisted in, with such additions as might be called for. The anæmic state of brain, in uræmic convulsions, and their frequent occurrence after the exhaustion of diarrhœa or vomiting, were urged as reasons for abstaining from depressing remedies. A case was cited in which, under these circumstances, small doses of opium had been used successfully. A case was also given in which acute pleurisy had passed off under the use of only local measures. The paper professed to deal only with the albuminuria of childhood.

Dr. Fuller had had opportunities of witnessing the author's treatment, but chiefly in adults, in whom it was not so successful as in children. In adults the renal affection was of longer standing. He had tried it in a child in private with success. It was successful in the dropsy of adults, but not so uniformly so as in dropsy in children after scarlet fever. If Dr. Dickinson's views were correct it was easy to understand that it should be more useful in children than in adults.

Dr. Basham said there were many points of practical value in the paper, and yet one or two things for comment. First, in cases where the renal affection followed scarlet fever, let the treatment be what it might, the majority of patients did well. No doubt the diuretic influence of water was beneficial, but he thought the theory brought forward was open to a difference of opinion. He ventured to think that there was not merely a blocking up of the tube, but also some change of the gland-cells—they had become effete cells, and accordingly were thrown off, and these, of course, obstructed the tube. But we know that nature tries to get rid of them, and no doubt diluents are of help in this way. He should, however, think the natural tendency of such cases to do well had more to do with recovery than the treatment.

Dr. Hillier had tried Dr. Dickinson's plan, and had found it scarcely so satisfactory. He (Dr. Hillier) agreed with Dr. Basham, that such patients, however treated, if seen early, would nearly always do well. His plan of treatment was to keep them in bed, to use hot-air baths, to give purgatives and good diet. Occasionally he had under care a severe case, and then had tried the water plan, but had been obliged to resort to other means. Perhaps, however, he had been too timid. In one case purpuric symptoms came on. There was considerable hæmaturia and epistaxis. With the water treatment the patient did worse, but when it was given up and gallic acid was administered, the patient recovered.

The Author, in reply, said that he was quite aware that the majority would get well if left alone, but a great number would not. He had been able to collect a large number of cases of deaths in the post-mortem books of St. George's Hospital. In reference to Dr. Basham's remarks, he said that the first changes were, he believed, in the quantity and not in the quality of the epithelium. In refe-

rence to Dr. Hillier's want of success, he would fall back on the explanation which Dr. Hillier himself had suggested, viz., that the treatment was not persevered in. The only fatal case in the paper was the one in which the vapor bath had been used.

Mr. Bainbridge asked if the treatment would have been equally successful without the digitalis? He had seen many such cases during the last month, and the patients generally were thirsty, and, as a consequence, without any special direction, drank a great deal of water.

The Author said that the water treatment had been tried alone in three cases.

Dr. Stewart related a case of dropsy following scarlet fever occurring in a woman who had recently been confined. Suddenly convulsions came on, followed by unconsciousness. Cupping the loins, and other measures, were followed by success, and he thought it a case in which the use of diluents would not have led to an equally favorable result.

Dr. Basham said that he thought that if any pathologist well versed in the use of the microscope would examine the gland cells of the kidney of a child who had died by accident, and then those of one who had died from renal disorder, he would find a well-marked difference. In the latter the cell was cloudy, and the nucleus obscure. It had become an effete and imperfect cell.

Dr. Fenwick said that he had examined the mucous membrane of the stomach in scarlet fever patients, and had found changes in the epithelium there as well as in that of the kidney.

In reply, Dr. Dickinson said that no doubt there was a change in the cells, but not primarily.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON: THURSDAY, SEPTEMBER 29, 1864.

Bantingism.—Everybody must have heard of stout Mr. Banting by this time, and many have read his almost pathetic pamphlet, wherein the adventures of this fat gentlemen in search of some one to relieve him of his burden of obesity, and his happy triumph at last, are related gratis for the benefit of his fellow-creatures. He was, it seems, 66 years old, about 5 feet 5 inches in stature, and weighed 202 pounds. He says he was unable to tie his shoe, or to attend to "the little offices humanity requires," without a good deal of difficulty. He was compelled even to go down stairs backwards to save the strain upon his joints, which he was obliged to keep bandaged, and could not help puffing and blowing on any slight exertion. He never found a seat large enough for him in an omnibus or at the theatre; moreover, the multitude would laugh at him, and he appears to have

been wanting in that non-conducting shield of insensibility and jollity which generally surrounds the real fat man. He was miserable, in fact, and did almost everything to "accomplish the great end of stopping and curing obesity." He starved himself, took to boating, tried sea-air and bathing, swallowed, as he says, gallons of liquor potassæ and other physic, rode on horse-back, went through a course of Turkish baths, consulted a host of physicians, &c. &c., but all in vain. At last he found the right man, who told him he had been eating too much saccharine and amylaceous matter, and was wise enough to specify what articles he should abstain from. His dietary plan was as follows:—

"For breakfast, I take four or five ounces of beef, mutton, kidneys, broiled fish, bacon, or cold meat of any kind except pork; a large cup of tea (without milk or sugar), a little biscuit, or one ounce of dry toast.

"For dinner, five or six ounces of any fish except salmon, any meat except pork, any vegetable except potato, one ounce of dry toast, fruit out of a pudding, any kind of poultry or game, and two or three glasses of good claret, sherry or Madeira—champagne, port and beer forbidden.

"For tea, two or three ounces of fruit, a rusk or two, and a cup of tea without milk or sugar.

"For supper, three or four ounces of meat or fish, similar to dinner, with a glass or two of claret.

"For nightcap, if required, a tumbler of grog—(gin, whiskey or brandy, without sugar)—or a glass or two of claret or sherry.

"This plan leads to an excellent night's rest, with from six to eight hours' sound sleep. The dry toast or rusk may have a tablespoonful of spirit to soften it, which will prove acceptable. Perhaps I did not wholly escape starchy or saccharine matter, but scrupulously avoided those beans, such as milk, sugar, beer, butter, &c., which were known to contain them.

"On rising in the morning, I take a tablespoonful of a special corrective cordial, which may be called the Balm of Life, in a wineglass of water, a most grateful draught, as it seems to carry away all the dregs left in the stomach after digestion, but is not aperient; then I take 5 or 6 ounces of solid and 8 of liquid for breakfast; 3 ounces of solid and 8 of liquid for dinner; 3 ounces of solid and 8 of liquid for tea; 4 ounces of solid and 6 of liquid for supper, and the grog afterwards, if I please. I am not, however, strictly limited to any quantity at either meal, so that the nature of the food is rigidly adhered to."

The result of this treatment we will give in his own words:—

"I have not felt so well as now for the last twenty years. Have suffered no inconvenience whatever in the probational remedy. Am reduced many inches in bulk, and 35 pounds in weight in thirty-eight weeks. Come down stairs forward naturally, with perfect ease. Go up stairs and take ordinary exercise freely, without the slightest inconvenience. Can perform every necessary office for myself. The umbilical rupture is greatly ameliorated, and gives me no anxiety. My sight is restored—my hearing improved. My other bodily ailments are ameliorated—indeed almost passed into matter of history."

Later still, at the end of the year, his weight was reduced 46 lbs.

and his girth 12¼ inches, and thus he expresseth his gratitude and thanksgiving:—

"It is simply miraculous, and I am thankful to Almighty Providence for directing me, through an extraordinary chance, to the care of a man who could work such a change in so short a time. Oh! that the faculty would look deeper into and make themselves better acquainted with the crying evil of obesity—that dreadful tormenting parasite on health and comfort. Their fellow men might not descend into early premature graves, as I believe many do, from what is termed apoplexy, and certainly would not, during their sojourn on earth, endure so much bodily and consequently mental infirmity. . . . . Most thankful to Almighty Providence for mercies received, and determined to press the case into public notice as a token of gratitude."

Since the publication of his pamphlet,* Mr. Banting has become a famous man, and his system is now the fashion, not only in England, the land of gross feeders, but even in the rarefied air of New England, where a fat man is seldom seen. There is nothing new in this method; it has simply been applied in an individual instance and made notorious. It is the single case which the public always does jump at, without regard to general principles or consequences, and Bantingism will unquestionably be popular for a time among fat men and women. It will undoubtedly in most cases produce the desired effect, so long as it is faithfully followed, and so far it may be considered a success, but there is another point to be regarded, which may not suggest itself to its disciples, viz., how far it is a safe method. Nature did not fill her cereals, fruits and edible bulbs with starch and sugar, nor her milk with sugar and fat, for no purpose. These elements play an important rôle in the animal economy, and although their use in excess may more than fulfil the desired object and cause their products to be accumulated to the temporary inconvenience of the individual, complete abstinence from them must lead to serious results. We would caution the obese, therefore, against any such experimentation without first consulting their physician. Mr. Banting's end is not yet, for he has given us the experience only of months. He may yet write an equally pathetic confession in the character of Cassius, and sigh that he is no more a Falstaff. We commend to the notice of the votaries of Bantingism the following judicious remarks of Dr. Radcliffe upon this subject, before the recent meeting of the British Medical Association, in the London *Medical Times and Gazette*:—

"Mr. Banting's rule is to take abundance of lean meat, claret, sherry, Madeira and tea, and to abstain as much as possible from bread, butter, milk, sugar, beer, and potatoes—articles containing starch and saccharine matter—to abstain as much as possible from the articles upon which Mr. Banting had lived almost exclusively in the days when he was a victim to obesity. The diet in Bantingism, indeed, is essentially the same as that prescribed in training for the ring or for the boat race. The chief peculiarity in each case is to allow a large amount of lean meat, and if there be any difference it is that the Bantingist deals more liberally with himself in this respect than the athlete. What Mr. Banting allows himself is rather more than is eaten by the averaged-sized man in training. It may be expected,

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* Letter on Corpulence. Mohun, Ebbs and Hough. New York.

therefore, that the known results of the process of training may throw some light upon some of the consequences of Bantingism. It may be expected that Bantingism cannot be carried beyond a certain point with advantage. In training this is certainly the case, for after a certain time, longer or shorter as the case may be, after within four months at the longest, the person in training rapidly gets "out of condition." Nor is it otherwise with Bantingism; at any rate, Dr. Radcliffe says that he has met with several persons who, after trying Bantingism for a while with no disadvantage, have thus got "out of condition" in an unmistakable manner, some of them becoming very gouty, and all of them experiencing a decided failure in strength and spirits.

"Dr. Radcliffe is disposed to look upon this loss of "condition" in these two cases of training and Bantingism as depending partly upon excess of nitrogenized food, and partly upon deficiency of fatty matter. The nitrogen of the food escapes in the main by the kidney as urea or uric acid, and if the kidney be not up to the work the system is apt to become gouty from the accumulation of these products in the blood. Hence it is not difficult to see how excess of nitrogenized food may, sooner or later, lead to ill health, and that especially in the case in which a proper amount of exercise is neglected. Nor is it difficult to see how deficiency of fatty matter may tend to bring about the same result, if, as Dr. Radcliffe supposes, a certain quantity of fatty matter be necessary to the proper nourishment of nerve-tissue. It is not difficult to see that nerve-tissue, which contains a large quantity fat, may be starved if the food do not contain a sufficient quantity of fat, and that this starving of the tissue may involve a corresponding want of nervous energy. At any rate, Dr. Radcliffe is disposed to look upon the diet in training and in Bantingism as calculated to nourish the muscles rather than the nerves, and he believes that this may be one reason why prize-fighters, like Heenan, have often been so seriously wanting in the power of sustained action, and why the followers of Mr. Banting have after a while begun to flag in the spirit which animated them at first.

"In a word, Dr. Radcliffe considers that it is not safe to ignore the old standards of food so completely as is done in Bantingism. He thinks that milk and bread are still typical articles of food. He gave reasons for believing that the farinaceous, and saccharine, and oily articles of food are, in proper proportion, not to be dispensed with without risk, inasmuch as they are the most suitable fuel for keeping up the heat of the human body at the proper point; and he gave reasons also for doubting whether saccharine and farinaceous matters in excess have the same tendency to favor the formation of fat as oily matters in excess. He is of opinion that certain persons may incline to the type of vegetable feeders rather than to the type of animal feeders, and that these persons may find the nitrogenized part of their food better in the albumen, fibrine, and caseine of vegetables than in the albumen, fibrine, and caseine of animals."

THE LATE DR. SILAS BROWN.—Died, in Wilmington, Mass., Sept. 15, 1864, Dr. Silas Brown, aged 84 years and 10 months.

Dr. Brown was born in Tewksbury, Mass., in 1779; received his

academic education at Phillips Academy in Andover, and studied medicine with Dr. Thomas Kittredge, of the same town. He began his professional labors at 21 years of age, in Kittery, Me., where he resided nine years, when, in consequence of failing health, he removed to Methuen, Mass. After a residence of eight years in this place, he removed to Wilmington, where he resided until his death, a period of forty-six years. He was admitted to the Mass. Medical Society in 1821, and remained an active member until his age entitled him to become a retired member, in accordance with the rules of the Society. During the long period of sixty-three years, from the time he commenced practice in Maine until his death, as his health would permit, he was ready to respond to the call of distress; his last visit, as medical adviser, being to a neighbor during the past summer.

Agriculture had been a theme of much pleasure to him for many years. His contributions to the *Boston Cultivator* and *New England Farmer*, for nearly twenty years past, may be found in the files of those papers.

He has lived a long and useful life, retaining his faculties to the last, and has now been "gathered to his fathers" "like a shock of corn fully ripe." B.

Foreign Intelligence.—Prof. Hyrtl has been chosen Rector Magnificus of the Vienna University for the coming year.

Prof. Scanzoni is about to leave Bavaria and take up his residence in Baden-Baden.

Prof. Langenbeck has been elevated to the rank of the nobility for his services in the late Schleswig-Holstein war.

A severe case of strychnia-poisoning is reported to have been cured by the use of worara in Königsberg.

At the last meeting of the Académie des Sciences, M. Wöhler, of Göttingen, was elected foreign associate in place of the late Prof. Mitscherlich. The other candidates were De la Rive, Geneva; Agassiz, Boston; Airy, Greenwich; Bunsen, Heidelberg; Hamilton, Edinburg; Martius, Munich; Murchison, London; and Struve, Pultava.

In a letter from Paris appearing in the *Wien. Med. Woch.*, it is stated that there are about twenty Germans practising in Paris, some of them enjoying a first-rate celebrity. The liberality with which all obstacles have been foregone by the French Government and Faculty is in striking contrast to what prevails in Vienna, where even the diplomas of different Universities of the same empire are not acknowledged as giving a right to practise. Among the German practitioners in Paris, the names of Gruby, Liebreich, Mandl, Meding, Sichel, and others are widely known. *Gruby* was a poor student at Vienna, who labored diligently under Hyrtl, Rokitansky, and other able professors, and he would probably have settled in his place of education, but being a Jew, all posts were denied him by Austrian intolerance. He therefore went to Paris, where he has acquired fame both as a teacher and practitioner. In the latter capacity his views are narrow enough, ignoring the maxims of medical science, and professing to treat disease solely by regimen. Still he is in enormous repute, his consultation room being crowded all day, chiefly with hysterical women, who often send their servants before hand to secure their turns. Gruby is a very

different man among his anatomical and chemical preparations at his laboratory at Montmatre, and his hospitality to foreign visitors is boundless. *Liebreich*, the former assistant of Von Graefe, at Berlin, has advanced far more rapidly than most practitioners at Paris. He has established an ophthalmic clinic, with sixteen beds and Dispensary, in the Quartier Latin; and here he not only sees the poor gratuitously, but gives the five-franc consultations to those of slender means in an adjoining room. This clinic is amply supplied with patients, and foreign physicians from all parts of the world flock to his lectures. His ophthalmoscopic demonstrations are skilfully performed, and he is always ready to enter into explanations. In the afternoon, the richly decorated waiting-room of his residence in the Champs Elyseés is frequented by rich and fashionable patients; for, combining most polished manners with profound knowledge of his specialty, he has acquired a large practice in a remarkably short time. Mandl also enjoys a good reputation both as teacher and practitioner, the laryngoscope especially of late occupying his attention. With a good consultation practice, and his Dispensary in the Quartier Latin, he yet finds time for the scientific pursuits which have given him an European reputation. Besides Liebreich, there are three other pupils of Von Graefe who are making their way in Paris as oculists, and Von Graefe himself always passes a month every year in that capital, during which he of course sees many patients. A Dr. Lowenberg, a pupil of the celebrated Poltzer, has also established himself as an aurist in Paris, and founded a clinic for diseases of the ear.—*Med. Times and Gazette.*

Reprint of the London Lancet.—The English edition of the *Lancet* being now almost excluded from this country by heavy duties and rates of exchange, the American reprint of it must be more valued than ever. It is published in New York by Mr. J. Herald at $5 per annum. Any one wishing to subscribe for it in connection with the Boston Medical and Surgical Journal, may receive the two works, as published, by addressing the publishers of the latter, at the rate of $7.50 per annum, in advance.

VITAL STATISTICS OF BOSTON.

For the Week ending Saturday, September 24th, 1864.

DEATHS.

	Males.	*Females.*	*Total.*
Deaths during the week - - - - - - - -	57	41	98
Ave. mortality of corresponding weeks for ten years, 1853—1863,	49.6	45.0	94.6
Average corrected to increased population - - - - -	00	00	104.0
Death of persons above 90 - - - - - - - -	0	0	0

Died,—At Somerville, 24th inst., Dr. George Bartlett, of Boston, aged 57 years.

Deaths in Boston for the week ending Saturday noon, Sept. 24th, 98. Males, 57—Females, 41.—Accident, 3—apoplexy, 2—disease of the brain, 4—inflammation of the brain-1—bronchocele, 1—cancer, 1—cholera infantum, 4—cholera morbus, 1—colic, 1—consumption, 18—convulsions, 4—croup, 4—diarrhœa, 12—diphtheria, 4—dropsy, 2—dropsy of the brain, 3—dysentery, 5—erysipelas, 1—scarlet fever, 2—typhoid fever, 2—typhus fever, 1—hæmatemesis, 1—disease of the heart, 1—indigestion, 1—infantile disease, 3—insanity, 1—disease of the kidneys, 1—disease of the liver, 3—congestion of the lungs, 1—inflammation of the lungs, 2—marasmus, 2—old age, 1—paralysis, 1—puerperal disease, 1—scrofula, 1—smallpox, 1—inflammation of the throat, 1.

Under 5 years of age, 45—between 5 and 20 years, 10—between 20 and 40 years, 24—between 40 and 60 years, 14—above 60 years, 5. Born in the United States, 71—Ireland, 21—other places, 6.